May 22, 1952

Dear Spicer:

You may be interested that I have just received and endorsed your WHO fellowship application. I note that you are applying for a tehure of 3 months. This is somewhat on the brief side, but in view of personal repponsibilities, I can well understand it. I was a little surprised at the penurious scale of the stipend for a candidate of your maturity, or is this supplemented by your own organization? If not, I should be interested to know whether you would be barred from accepting auxiliary financial support.

In the endorsement, I indicated that you would be welcome at any time during the next academic year, except that I shall be away during February 1953. October-November-December, or perhaps slightly better, November-December-January will do very well. As soon as you have settled your definite plans, I should like to know just when you plan to arrive.

It is probably not too soon to begin detailed discussions of research problems. From our experience, I would conclude that a systematic study of related strains is more likely to be fruitful than a more extended survey. It seems very likely that groups B and D form one connected system, within which transductions will be freely possible, limited only by the suitability of available phages. Mr. (soon Dr.) Zinder is working to consolidate this principle. The most logical further step is the examination of the remaining major groups, especially C. If the mechanism of genetic exchange can be worked out there, we can then try to relate, if possible, the behavior of the different groups. Perhaps I am overoptimistic, but I am fairly confident that we can find what we are looking for in these groups, by following the methodology already established. For this reason I suggest that we study a number of strains of a single serotype whose phages have been well defined, and I conclude that Williams-Smith has already done this kind of preparatory work with S. thompson. We ourselves spent over two years "preparing auxotrophs...of the common types [planning to] follow up any cross that seems particularly interesting or hopeful", but it was not until we undertook a concentrated study of S. typhimurium that interpretable results were achieved. May I urge as strongly as I can that we follow a similar course here; if another type besides thompson seems preferable, let me hear. Once the technique of intra-strain recombination is validated, it should be very easy then to proceed with inter-type studies, with the main objective, I suppose, of serotypic recombination.

Not long ago we received a sample of Boulgakov-Serbic's "flagellar phage", and have been using it to select O-forms of varying stability, which have been interesting for our genetic analysis of the flagella. We find that very few artificial O-forms are completely stable, and have been surprised at the ease with which OH-"reversions" have been selected even from such strains as Felix(S. typhi O-901. Have you had any experience with this?

Sincerely,

Joshua Lederberg
*Associate Professor of Genetics

*P.S. Professor Tatum was my counselor for my Ph.D., but went back to Stanford University from Yale.